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Interactive comment on "Weakly coupled atmospheric-ocean data assimilation in the Canadian global prediction system (v1)" *by* Sergey Skachko et al.

Anonymous Referee #2

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This paper describes the weakly coupled approach to data assimilation taken by ECCC for the initialisation of coupled NWP forecasts. The methodology is described in detail, and in particular compared to the uncoupled approach which is used as a control for assessment. Impact is measured by looking at observation statistics (analysis and forecast) and forecast error statistics.

The paper is overall of high quality, well written, and of scientific interest. There are no insurmountable problems with this manuscript, but I have one request which may be regarded as major, though I hope this could be addressed quickly.

Major point: The choice of forecast error statistic is one that I have not seen before

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and I have had long discussions with colleagues about its applicability. Specifically it is the choice of verifying forecasts against the mean analysis of the two experiments. Starting with an example, if forecast 1 matches exactly analysis 1, then it will verify worse than if the forecast 2 drifts towards analysis 1, as it will approach the mean of analysis 1 and 2.

Looking in more detail, say at Simmons and Hollingsworth, 2002 (https://doi.org/10.1256/003590002321042135), differencing the equation for forecast errors on pg 668, you see that for this to be a true measure of forecast error differences then you are assuming that the true forecast errors times the correlation between the true forecast error and the mean analysis is equal for both forecasts ($f_{1T}c_{f_1a} = f_{2T}c_{f_2a}$). So then you have the additional problem of looking at how forecast 1 correlates with analysis 2, and vice versa. I think this just muddies the waters here!

The clean solution to this is to use an independent analysis for verification. Indeed, page 9 line 29 says that you have already produced such plots using ERA5. I would suggest to replace the results you show with those verified against ERA5 to simplify the interpretation of your results.

Minor points: Page 2, line 4: worth referencing ECMWF here: P. Bauer and D. Richardson. New model cycle 40r1. ECMWF Newsletter No. 138 - Winter 2013/2014, (138):3, 2014. URL https://www.ecmwf.int/node/14581

Page 4, line 14: are the increments computed on the full 80 levels? please clarify.

Page 7. line 26: "The daily ocean SAM2 DA (Sect. 2.2) assimilating only SST data is computed at 0000 UTC." Is this a daily mean SST field, or is it valid at 0000? Please clarify.

Page 8, line 10. Please could you clarify if the ensemble used in the 4D-EnVar uses a coupled or uncoupled model?

Page 8, line 12:14. "However, by saving the atmospheric fields from the 6-h coupled forecasts and using these to force the ocean model, this is equivalent to the explicit use of the fully coupled atmosphere-ocean-ice model." My understanding of this line is as follows: "However, by saving the atmospheric fields from the 6-h coupled forecasts and using these to force the ocean model, this is equivalent to the explicit use of the fully coupled atmosphere-ocean-ice model with a 6 hour coupling frequency". Is this a correct reading? If so, should it be added for clarity?

Page 9, line 2: The test period used here of 2 months is short. Specifically it might be too short to see any major changes in the ocean component. Given the computational cost of the coupled assimilation experiments it would be unreasonable for anyone to ask for an extended period of testing. I think, however, this warrants a comment in the conclusion to reflect that the results should be viewed in this context.

Page 9, line 6:7. "Differences between these two systems are expected for the SST as well as for near-surface layers in both atmosphere and ocean models." This sentence I spent a while trying to understand what may be very obvious to the authors, and in the end I cannot see why SST is expected to be different in the two systems. I thought the SST analysis described in 2.3 was independent of any model, and so should not be different in the analysis of the weakly coupled or uncoupled systems. Perhaps this refers to forecasts of SST? Please can you expand on this to make your point more explicitly.

Page 10, line 7:8. "The OmF standard deviations produced by CPL are systematically lower than those produced by UNCPL in all three regions." This may be systematic, but it a is very small difference.

Page 12, line 24: "an integrated software" -> "integrated software"

Figures 6, 7 (top), 9, 10, 11: please state in the caption and in the text that these are plots of errors, not just std etc.

Figures 13 and 14: The grey colour looks blue which is misleading. Maybe replace the

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orographic shading with a constant colour and make the grey areas that same colour.

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